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VII. — *Stampini and Pascal on the Catullus Manuscripts*

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IN spite of intended brevity, a short introduction is necessary.

It is agreed that there was in the fourteenth century a manuscript of Catullus in the Cathedral Library of Verona, and that it had probably been there from at least the tenth century. This is now lost.

Two existing manuscripts dating from the latter part of the fourteenth century (so far there is agreement) represent the "Lost Verona" with tolerable accuracy, though differing from each other at many points. These are G of the National Library in Paris, and O of the Bodleian Library in Oxford. O is undated, G bears the ostensible date of 1375.

Up to the nineteenth century, while editors repeatedly spoke of readings in manuscripts which they had used, there was no methodical exhibition of them. But about a hundred years ago, editors began to arrange and exhibit more adequately. For Catullus this improvement begins with Lachmann's edition (1829). G and O were not yet generally known.<sup>1</sup> Lachmann as a rule cites two manuscripts, and two only, D and L, characterizing them as manuscripts "with the one or the other of which all the rest of the uninterpolated manuscripts everywhere agree." Such is the explanation given. But the real reason was undoubtedly the fact that D and L were right at hand for Lachmann in the Royal Library of Berlin; for his own occasional citations from Parisinus 7989 (P) and Laurentianus 33, 12 show them to be better manuscripts. Lachmann does not give the date of any manuscript cited. That of D is 1463, or only about seven years before the first printed edition.

<sup>1</sup> G was known to Sillig, who gathered readings from it and other Paris manuscripts in 1823; and Valpy's edition, published seven years before Lachmann's, called special attention to O. But Lachmann missed both.

Other manuscripts were presently brought into service. Schwabe cited six, including G. Robinson Ellis, after working in many libraries, set up a great apparatus, using twenty-one manuscripts, including O and B, the latter dated 1412. In neither edition does he cite P, dated 1423 (the famous manuscript of the *Cena*), except for a very few scattered references to it under its library number. This silence will always remain strange; for a collection of readings made by Sillig from five manuscripts in the National Library in Paris was published by Roszbach in 1859, and yielded nearly six large pages of readings from P. Besides, there was Schwabe's second edition, which gave readings; and there was Paris close at hand.

But to proceed. Schwabe in his second edition used ten manuscripts. Schulze collated M (to which Ellis had drawn attention), and gave in his edition the readings of O, G, M, and D, on the theory that M and D best represented two masses of the secondary manuscripts which had come down from sources independent of O and G. This remained Schulze's position, as appears in his summary (*Jahresbericht*, 1920) of publications upon Catullus since 1905. And let me say at this convenient point that, in substance, Schulze's argument in this new summary is identical with Stampini's argument, which we are going to examine, and that Stampini's argument is in substance identical with that of a score of predecessors, including Ellis. The attempt is, in brief, to elevate a favorite manuscript, not exactly to equality with G and O, but to independence of them, so that any readings one likes to adopt from them may be regarded as having descended from the "Lost Verona Ms."

We have reached Stampini. In 1915 he gave, before the Royal Academy of Sciences in Turin, two papers on Catullus, which were published the next year in a pamphlet. In this monograph, Stampini relies entirely upon Ellis's apparatus, discussing and citing substantially the same manuscripts, and, like Ellis, having nothing to say about P.

His study was suggested to him by some added readings which Ellis in his small undated edition (1904) gave from a manuscript in Brescia. Let us call this manuscript the Brixianus.

Stampini's argument of 48 pages may be condensed as follows:

1. Brix. has many wonderful agreements (*mirabili concordanze*) with h of the British Museum. They are clearly from one original. To be sure they do differ. But the explanation is simple. Where this happens, the reason lies in errors or ignorance, largely, though not wholly, due to paleographical grounds, especially misunderstandings of abbreviations.

2. There are places where they have a good and acceptable reading in common with another manuscript, or with a very few manuscripts, or with a few. But they also differ elsewhere from this same manuscript or these same manuscripts. The explanation is as before, in the copying of their exemplar from its exemplar, or of the exemplar of these other manuscripts from their exemplar.

3. They occasionally agree with O, while not passing into the field of G.

4. They have many good and acceptable readings, not due to interpolation, which are peculiar to them and are not in O.

Therefore we must believe that these manuscripts have come down, independently of O and G, from the "Lost Verona Ms." And their readings become available for the restoration of the text.

Pascal, in his recent edition of Catullus (1916), substantially accepts Stampini's reasoning. He also, because of their excellence, gives many readings from d and Cuiac., while holding his ultimate judgment in reserve.

Before we pass to a critical examination of Stampini's reasoning, let me say that his phrase "wonderful agreements" should not be allowed to overawe us. There is nothing extraordinary in the close general agreement of two manuscripts. I can show the same general agreement in a group of three

manuscripts, or of six, or even of seven. Thus Stampini speaks of the Hamburg Ms., H, as having *destinat* in 21, 12, where O and G have *desinat*. But I can show five more manuscripts that agree with the Hamburg Ms., not only here, but pretty generally.

Now for the criticism proper :

1. In assuming paleographical blundering as the principal cause of variation in manuscripts, Stampini is certainly wrong. Thus in the place just referred to, where all editors have long read *desine*, the manuscripts give us *desinat* (O G), *desinas*, *desine*, *destinat*, *destituat*, *desideat*. These differences are obviously not due to paleographical confusion. Only one other explanation is possible: they are due, except the inherited original reading, to deliberate attempts to mend an obviously corrupt text. Put mathematically, we have here  $83\frac{1}{3}$  per cent of emendation.

2. In saying that good and acceptable readings in the combination Brix. h are not due to interpolation, that is, attempted emendation, Stampini is begging the principal question, like Schulze after him and many a man before him. Schulze says (*op. cit.*, p. 19): "The scribes were generally glad enough if they could merely make out Latin words, whether these yielded sense or not." But it is exactly here that proof is wanted. The odds are against the explanation of independent descent for these good readings; for it is a very suspicious circumstance that, toward the end of the manuscript period and the beginning of the period of print, the readings steadily improve. And it is precisely for these later manuscripts and readings that Stampini, Schulze, and others are most vigorously fighting!

3. There are other manuscripts which, from time to time, agree with O without agreeing with G, or with G without agreeing with O. That is the case with D, which, a number of times, agrees with G against O; it is the case with Ms. a, of the British Museum, which, in spite of resemblance to D, occasionally agrees with O against G. It is the case with L,

which, in additions by the first hand, has many agreements with O. It is strikingly the case with P, the manuscript with which Stampini seems to have no acquaintance. P has more agreements with O against G than Brix. and h have. If we proceed by Stampini's reasoning, we shall have the heavens studded with these independent luminaries !

4. Stampini's argument assumes that readings in common between O and Brix. h must have come down independently from a common ancestor. This is based on what has been, and still seems to be, the prevailing theory of descent in a straight line : A begets B, B begets C, C begets D, etc. But it has begun of late to be recognized that crossings of traditions take place, and in the case of a few authors stemmata have been published which connect one archetype with another. I early felt that I recognized that, in greater degree indeed than any one has yet suspected for any author, the manuscripts of Catullus were crossed and recrossed, and not archetypes alone, but individual manuscripts, so that scarcely one after the fourteenth century has come down unmixed. No stemma of the Catullus manuscripts from the earliest time down to Schulze's article, inclusive, hints at such a thing, and no programme or prolegomena propose it — in spite of the fact that, in a postscript in G, the scribe himself begs the reader's pardon for its inaccuracies, and excuses himself on the ground that he had no other manuscript by which to correct it !

5. Stampini suffers, without being clearly aware of it, from the same difficulty from which all the editors have suffered, a very limited knowledge of the manuscripts. I can easily illustrate by saying that another manuscript which he evidently does not know, the Brera Ms. in Milan, forms a third in the group to which his combination Brix. h belongs, and represents the family at a point one generation farther back. Most dramatic fact of all, Stampini's group owes much of its character to crossing from another manuscript of which, probably from following Ellis, he has no knowledge — I mean that

same P of the National Library in Paris. Even the highly individual titles in Stampini's group, of which he makes much, are evidently derived from the titles in P. To illustrate further: In Stampini's discussion, he gives many readings peculiar to Brix. h, or shared only by a very few (*pochissimi*) or a few (*pochi*) — "in general not to exceed four." Thus for *vetitabas* in place of *ventitabas*, 8, 4, he adds "with H." But I can give seven more manuscripts with this reading. For the inversion *puelle ferunt*, 2, 11, he gives no additional manuscript; but I can give twenty-two. For the corruption *nemo sinunt*, 12, 13, he gives no additional manuscript; but I can give twenty-four. Now if it is a good thing to know *something* more about the Catullus manuscripts (say about Brixianus) there would seem to be a hope of considerable advantage in a *complete* command of them. And here is one of the major points which mark my own studies in the Catullus tradition.

I should like now briefly to show, in contrast with the methods I have been criticising, how I came to be able to enlarge other people's citations, and how I am using my materials.

I early felt that the relations of the Catullus manuscripts had not been made out. But the published readings were so intermittent and scattering that there could be little hope of a solution through their aid alone. Even today, there are complete published collations of only two of the manuscripts, O and G, with an approach in the case of M, D, and L.

Still, I believed that I divined two things. One was that the great mass of the manuscripts outside of O and G had come down from a common third source, now lost. The other was, as I have already said, that the secondary manuscripts were heavily crossed: for there was no trouble in finding evidence for quite contradictory hypotheses.

Going to Rome as Director of the American School of Classical Studies, in its first year, 1895-1896, I seized my opportunity to look at the Catullus manuscripts in the Vatican

Library, in the hope of bridging over some of the existing chasms. A number of our students had taken an introductory course in paleography at the Vatican, and, in consequence, I was able (through the great kindness of Father, now Cardinal, Ehrle) to get the privileges of the manuscript working-room for four of them who wanted actual practice. They collated, under my direction, four manuscripts, selected by me out of the twelve entered in the three catalogues in which the name of Catullus appears. Heyse, in 1855, had said there were eleven. Eleven of the twelve duly came to me at my request; but, for the twelfth, a Greek author came instead. I was precisely where Heyse must have been, and probably many another scholar before his time and after it. I rang the changes on the figures of the catalogue number, but in vain. I was strongly tempted to give the matter up. It was late, in a year of great responsibility and hard work. The manuscript, if there really was one, was probably of little value in any case. But I could not bring myself to let go. After a long series of experiments, I got the manuscript, quite simply at the end. When it came to me, I at once thought it probable, and at my next sitting, after preparing special tests, felt morally sure, that I held in my hands that which I had believed had once existed, but had never guessed that I, or any other man, should see — the peer of O and G, and the original of the greater part, at any rate, of the whole mass of secondary manuscripts.

I christened the manuscript R, and collated it. On my return to America, I offered a seminar in the subject, and found the material insufficient. Then Mr. C. E. Dixon volunteered to return to Europe and make complete copies of manuscripts which I especially wanted, B, P, A, and V. A little later, Mr. O. M. Washburn contributed copies of Dresd. and Urbinas 641. I also had complete copies made of D and L, and had a photograph made of Ms. a in the British Museum. I could now control twenty manuscripts.

Ellis and Schwabe, in giving a reading, arrange in alpha-



betical order the symbols of the manuscripts having it. In contrast I made out hints of families, and had special paper for a collation printed and ruled, on which these tentative families appeared. I was not, in fact, far wrong. But after I had amassed a complete collation of my twenty manuscripts, I found it still insufficient. I then determined to get a complete collation of every existing manuscript of my author. I raised Heyse's guess of a total of seventy (he enumerates only forty-three) to an actuality of a hundred and fifteen or so, and published the list. Mr. B. L. Ullman, then fellow of my university, and a member of my Catullus seminar of that day, was allowed to come abroad to help me. With the assistance of Mr. Evan T. Sage, who happened to be at hand, and could give his time for a while, we dealt with every known Catullus manuscript.

The mechanical difficulties in collating so many manuscripts, especially of a text so corrupt, were great. Any manuscript would differ in multitudes of places from any modern printed text, and the number of entries one would have to make in collating under the ordinary system would be overwhelming.

A better way occurred to me. I had a book of a hundred and fifty copies privately printed, with the text which, on the basis of O G and R, I believed the "Lost Verona" to have had. The manuscripts would obviously differ much less from this than from a modern text. Armed with one of these copies, one of us would sit with pen and ink before a manuscript, and make the printed book conform to that manuscript. About a vast number of readings, nothing would need to be noted. Moreover, when I came to my work of comparison later, I should have for each manuscript the single book alone to deal with, instead of a modern text *plus* a notebook. My book would be for me the manuscript itself. And the fact that, all my books being of the same size and shape, they would pack, and handle, in smooth masses together, all open, if I wished, at the same page, and always exhibiting a given

passage at the same point in that page in every one, would facilitate comparing and recording. Yet I beg you not to think the labor slight.

There is no time in which to describe my processes. I can only speak of the provisional results, first for the text of Catullus, and second, for our general conception of the way in which fifteenth-century scribes and owners worked.

My large result for Catullus was divined as probable, and published as such, before I began to get the complete manuscript material before me. I am finding what I foresaw: all the manuscripts except O G R are derived from these three, and principally from R. This being so, we may and must cut off the whole web below the manuscripts O G R. In these three we possess the whole of the genuine tradition. Moreover, the work of G<sup>2</sup>, that is, the first corrector of G, does not belong in this genuine tradition, for it was done from M, a copy of R, by a man who never set eyes on the original of G, and forty years, at the least, after G was written. The text of Catullus must accordingly be based entirely on O G R and R<sup>2</sup>. R<sup>2</sup> was the owner of R, Coluccio Salutati (1330-1406), Latin Secretary of the Republic of Florence, a scholar, and a great collector of Latin manuscripts. We have throughout, of course, the delicate task of distinguishing between what he preserved to us, which is of incalculable value, and what he changed or added; for there is no question that methodical emendation is already present in his work (in the fourteenth century, please notice) not only in R, but in other manuscripts which may be identified as having been owned and worked over by him.

G and R are sister manuscripts, copied from a copy of the "Lost Verona MS.," made for Coluccio, pretty surely in 1375.

I have not yet published a collation of R, because it has proved to be difficult to persuade people of its character, though I have given strong demonstration. Ellis agreed with me, and Merrill does; but Friedrich and Schulze found

R of no value. I have waited until I could command the evidence of the whole mass of Catullus manuscripts. This obviously should come out with the collation, rather than after it.

The work is well advanced. I have my general stemma. I know, for example, where Stampini's group comes from (it is a composite from four sources), where D comes from, where the Hamburg Ms. and the C group come from, where other groups that have never been suggested come from. I know a good deal about the crossings, though not yet everything, and a good deal about the stemmata inside the groups, though not yet everything.

It is a good while since I began. But the task I set myself is not only perplexing in the highest degree, but enormous in extent. And there have been other things that claimed my time. The continuous attention which such a piece of work calls for by its very nature was impossible under the conditions of university teaching. But now, with good fortune for a few years, I hope to finish the undertaking. I can already vouch for three points with regard to fifteenth-century manuscripts of any Latin classic, if, as we have the right to do, we judge from the Catullus group:

1. Most of the manuscripts are highly sophisticated. *There is no such thing as an uninterpolated manuscript.*
2. As a whole the manuscripts are *highly composite*.
3. One needs, for the establishment of certainty about the interrelations and respective values of the manuscripts of any author, to be equipped with *complete collations of all that have survived*.